



Philosophical Transactions

Please note: Due to an error in the print volume, the page numbering in this article may contain either page numbering skips, or page numbering repetitions, or both. However, the article content is presented in its entirety and in correct reading order.

Please click on "Next Page" (at the top of the screen) to begin viewing the article.

A Serie's of Quere's propounded by Mr. Isaac Newton, to be determin'd by Experiments, positively and directly concluding his new Theory of Light and Colours; and here recommended to the Industry of the Lovers of Experimental Philosophy, as they were generously imparted to the Publisher in a Letter of the said Mr. Newtons of July 8. 1672.

IN the mean while give me leave, Sir, to insinuate, that I cannot think it effectual for determining truth, to examin the several waies by which Phænomena may be explained, unless where there can be a perfect enumeration of all those waies. You know, the proper Method for *inquiring* after the properties of things is, to deduce them from Experiments. And I told you, that the Theory, which I propounded, was evinced to me, not by inferring 'tis thus because not otherwise, that is, not by deducing it only from a confutation of contrary suppositions, but by deriving it from Experiments concluding positively and directly. The way therefore to examin it is, by considering, whether the Experiments which I propound do prove those parts of the Theory, to which they are applyed; or by prosecuting other Experiments which the Theory may suggest for its examination. And this I would have done in a due Method; the *Laws of Refraction* being throughly inquired into and determined before the nature of *Colours* be taken into consideration. It may not be amiss to proceed according to the *Series* of these *Queries*; which I could wish were determined by the Event of proper Experiments; declared by those that may have the curiosity to examin them.

1. Whether rays, that are *alike* incident on the same *Medium*, have *unequal* refractions; and how great are the inequalities of their refractions at any incidence?

2. What is the Law according to which each ray is more or less refracted; whether it be that the same ray is ever refracted according to the same *ratio* of the sines of incidence and refraction; and divers rays, according to divers *ratio's*; or that the refraction of each ray is greater, or less without any certain rule? *That is*, whether each ray have a certain degree of refrangibility according to which its refraction is performed; or is refracted without that regularity?

3. Where-

3. Whether rays, which are endued with particular degrees of refrangibility, when they are by any means separated, have particular colours constantly belonging to them; *viz.* the least refrangible, *Scarlet*; the most refrangible, *deep Violet*; the middle, *Sea-green*; and others, other colours? And on the contrary?

4. Whether the colour of any sort of rays apart may be changed by refraction?

5. Whether colours by coalescing do really change one another to produce a new colour, or produce it by mixing only?

6. Whether a due mixture of rays, indued with all variety of colours, produces Light perfectly like that of the Sun, and which hath all the same properties, and exhibits the same *Phænomena*?

7. Whether the component colours of each mixture be really changed; or be only separated, when from that mixture various colours are produced again by Refraction?

8. Whether there be any other colours produced by refraction than such, as ought to result from the colours belonging to the diversly refrangible rays by their being separated or mixed by that refraction?

To determine by Experiments these and such like *Quære's* which involve the propounded Theory, seems the most proper and direct way to a conclusion. And therefore I could wish all objections were suspended, taken from *Hypotheses* or any other heads than these two; Of shewing the insufficiency of Experiments to determine these *Quære's* or prove any other parts of my Theory, by assigning the flaws and defects in my conclusions drawn from them; Or of producing other Experiments which directly contradict me, if any such may seem to occur. For if the Experiments, which I urge, be defective, it cannot be difficult to shew the defects; but if valid, then by proving the Theory they must render all Objections invalid.

So far this accurate Proposer; whose Method appearing to be most genuine and proper to the purpose it is propounded for, and deserving therefore to be considered and put to trial by Philosophers, abroad as well as at home; the Publisher, to invite and gratify Forraigners, was willing to deliver the above recited Extract of Mr. *Newtons* Letter in the language also of the Learned, as followeth; Z z z z 2 Ex-

Excerptum ex *Isaaci Newtoni* Epistola, nuper ad Editorem script, quâ ipse genuinam suggerit Methodum, doctrinam suam de *Luc & Coloribus*, antehac propositam, evincendi, subjectâ certorum *Quæstionum*, debitæ Experimentis solvendorum, serie.

Liceat mihi hac occasione tibi significare, nequaquam censere me, efficacem esse determinandæ veritatis rationem, quâ diversi examinantur modî, quibus *Phænomena* explicari possunt, nisi ubi perfectâ fuerit omnium istorum modorum *Enumeratio*. Nosti, genuinam proprietates rerum investigandi Methodum esse, quâ illa ab Experimentis deducuntur. Ac jam antè tibi dixeram; Theoriam à me propositam evictam mihi fuisse, non quidem inferendo rem ita se habere quia haud se habeat aliter, i. e. non eam deducendo duntaxat à contrariarum suppositionum confutatione; sed ipsam ab Experimentis, positivè & directè concludentibus, derivando. Vera itaque ratio eam examinandi hæc erit, si consideremus scilicet, num Experimenta à me proposita illas Theoria partes, quibus accommodantur, reverà probent; vel si alia prosequamur Experimenta, quæ ab ipsa Theoria ad examinandam eam suggerantur. Atque hoc ipsum Methodo genuinâ fieri velim; perveſtigatis primum ac determinatis Legibus Refractionis, priusquam Colorum natura disquiratur. Præter rem itaque haud fore crediderim, disquisitionem hanc ex sequentium Quæſitionum serie instituere; quæ quidem ut à solertibus sagacibusque natura Mystis pronuntiatis Experimentorum Eventibus, dirimantur, in votis quàm maximè habeo. Ea sunt;

Primò, Num radii, qui æquali incidentiâ in idem medium incidunt, Refractiones habeant inæquales; quantaque sint refractionum, quas illi subeunt, inequalitates in quavis incidentiâ?

Secundò, Quanam ea Lex sit, juxta quam radius quilibet magis minùsve refringitur? sinè, quòd idem radius semper refringatur secundum eandem rationem Sinuum Incidentiæ & Refractionis; diversi autem radii, secundum rationes diversas? An verò, quòd cujuslibet radii refractionis major minorve sit absque ulla regula certa? Hoc est, Utrum unusquisque radius certum habeat gradum Refrangibilitatis, juxta quem fiat ipsius refractionis; an verò refringatur sine ista regularitate?

Tertiò, Num radii, certis gradibus refrangibilitatis præditi, quando, quod demum cumque modo, secernuntur, certos obtineant colores ipsos proprios; puta radii minimè omnium refrangibiles, Coccineum; maximè refrangibiles, saturum Violaceum; intermedii, sub-Viridem; alii, alios? Et è contrà.

Quartò, Num color cujusvis generis radiorum seorsim existentium mutari possint Refractione?

Quintò, Utrum colores coalescendo reverà se invicem mutant ad producendum colorem novum; an verò eum producant nonnisi se invicem commiscendo?

Sextò, Num debita radiorum miscela, omnigenâ colorum varietate prædita, Lucem producat Solari luci simillimam, quæque easdem omnino proprietates obtineat, eademque Phænomena exhibeat?

Septimò

Septimò, *Utrum componentes cujusvis miscela colores reverà mutantur ; an verò secernantur duntaxat, quando ex mixtura illa varii colores rursus producantur per Refractionem ?*

Octavò, *Denturne ulli alii colores Refractione producti præter eos, quos oriri oportet à Coloribus, ad radios diversimodè refrangibiles pertinentibus, dum illi refractione istà secernuntur vel miscentur ?*

Per Experimenta determinare hæc similiaue Quæsitæ, quæ propositam Theoriam involvunt, maxime genuina directaque videtur ad Conclusionem via : Proindeque omnes velim Objectiones suspendi, quæ ab Hypothesibus desumuntur ullisve Fontibus aliis, quàm his duobus ; quibus nempe vel ostendatur Experimentorum ad determinanda hæc Continua probandasve nullas alias Theoriae meae partes insufficientia, hallucinationes defectusque in Conclusionibus meis inde deductis indigitando ; vel alia producantur Experimenta, è diametro mihi opposita, si quæ talia occurrere videantur. Si enim Experimenta, quæ à me urgentur, laborant defectibus, difficile haud fuerit eos ostendere ; si verò valida fuerint, eo ipso dum Theoriam meam asserunt probantque omnes Objectiones convellunt.

Some Annotations of the Learned Dr. Walter Needham upon a Discovery pretended to have been made by the famous Monsieur Pecquet of a Communication between the Ductus Thoracicus and the Inferior Vena Cava.

The Relation it self of that pretended Discovery, as it is to be found in the Journal des Scavans, of Feb. 8. 1672.

The Annotations of Dr. Needham.

THE Discovery made about twenty years since by M. Pecquet of the *Ductus Thoracicus*, seemed not sufficient to clear up all the Difficulties to be met with in the New opinion, which this Channel hath occasion'd, concerning Sanguification.

It might be said among other things, That there appears no reason, why Nature, which does nothing without design, should carry the matter of the Blood into the *Sub-clavials*, and thence make it descend by the Trunk of the *Vena Cava*, (A.) unless it be to keep the Chyle from entering all at once and altogether pure into the Heart, and that the mixture, which is made of the Chyle with the Blood along this way, may dispose the Chyle, by a kind of contagious fermentation the

(A.) I think the reason there mentioned to be very sufficient for the inserting of the Trunk of the *Ductus Thoracicus* into one place alone ; at least as good as any that are afterwards given to prove the contrary. For, all proofs of this nature are but loose conjectures at the best : the matter admitting of no other demonstration than what is ocular.

(B.) Till the Lower insertion be shewed, we are bound to believe, that Nature thought the single more